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RE: "PSYCHIATRIC DIAGNOSES IN HISTORIC AND CONTEMPORARY MILITARY COHORTS: COMBAT DEPLOYMENT AND THE HEALTHY WARRIOR EFFECT"

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Regarding a recent *Journal* article by Larson et al. (1), we agree with the authors about the importance of assessing the psychological costs of current military operations in Iraq and Afghanistan and about the need for psychometrically rigorous diagnostic methods for establishing incidence and estimating past history of disorder. Unfortunately, their study falls short of this measurement standard when it relies on diagnoses of unknown reliability derived from TRICARE medical records of military personnel. The results they obtain show lower overall incidence rates of all disorders in a cohort of US Marines deployed for combat in Iraq than in nondeployed Marines and other comparison samples. The incidence of only posttraumatic stress disorder (PTSD) is elevated in the deployed cohort regardless of whether those with predeployment disorders are included or excluded. However, the incidence rate is 1.6% (1.5% when prior PTSD is excluded), far lower than rates of "probable PTSD" estimated in previous research of troops serving in Iraq and Afghanistan that has relied on symptom screening scales (2).

Larson et al. (1) suggest that the main reason for their much lower rate of PTSD is that the screening scales used by other investigators are likely to identify many false posi-

tives. Although there is widespread recognition of limitations (e.g., false-positive case identification) associated with use of symptom scales to screen for PTSD in this military population, the discrepancy between incidence rates cannot be summarily attributed to this source. Equally plausible is sampling bias introduced into the Larson et al. study by reliance on a health-care-seeking population. That is, the authors' case finding does not include individuals who develop the disorder but do not present to TRICARE health-care providers. Moreover, in these settings, cases of PTSD are highly likely to go undiagnosed, as Toomey's (3) accompanying commentary points out. Accordingly, representative scientific sampling requires procedures independent of the selective impact of the health-care setting.

Larson et al. (1) argue that whatever cases are missed or misclassified by their case-finding procedures, the losses are not differential for the cohorts being compared. This assumption needs to be checked. It is possible, for example, that individuals selected for and involved in combat are especially sensitive to stigma that might compromise their warrior status in the eyes of their fellow Marines; if so, we would expect these individuals to be less likely to disclose

their symptoms or seek treatment for them than individuals in the comparison samples of nondeployed troops. To make their case that the presence of PTSD and other disorders is equally likely to be missed in the cohorts being compared, Larson et al. need to pay much more attention to the role of stigma when seeking treatment in military settings (2).

Larson et al. (1) present evidence for substantial rates of early separation from the military that, as they point out, may selectively screen for health as demonstrated by surviving the rigors of basic training. It would be useful to pursue this hypothesis by examining the actual role of psychiatric problems in relation to other possible factors in those affected by early separation compared with those who are deployed and serve in combat.

We hope that Larson et al. (1) will build, in several important ways, on their previously presented work. It would be especially important for them to conduct diagnoses independent of treatment status in combat deployed and comparison samples with special attention to selective factors that determine who does and does not enter treatment settings and, once there, who is and who is not accurately diagnosed with PTSD and other psychiatric disorders.

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